it without learning something new about the history of the series of inventions and discoveries which have culminated in Transatlantic Marconigraphy.

Catalogue of the Collection of Palaearctic Butterflies Formed by the late John Henry Leech. By Richard South, F.E.S. Pp. vi+229; portrait and two coloured plates. (London: Printed by Order of the Trustees of the British Museum, 1902.)

It is very gratifying to notice how frequently, at the present day, large private collections of objects of natural history, when of real importance, find their final resting-place in the British Museum, or in some other great public collection, where their treasures are available for ever, instead of being dispersed on the death of the owner, and by such dispersion alone, losing a large part of their scientific value, besides the probability of a considerable portion being neglected, and sooner or later lost or destroyed.

Especially is this the case with great special collections, like that brought together by Mr. Leech, at great expense, and with untiring energy and perseverance, from Lapland to Marocco and Algeria, and from thence to Cashmir, and from Cashmir to Japan, including the materials used in the preparation of his great work on the "Butterflies of China, Japan, and Corea," which is likely long to remain the standard authority on the subject. A great part of these collections was formed by Mr. Leech himself in his numerous entomological journeys, while others were procured for him by enterprising collectors like Mr. A. E. Pratt, in almost unknown and unexplored parts of Western China and Thibet. Besides these, Mr. Leech's collection includes (by purchase) the bulk of the collection formed by the late Mr. Henry Pryer, himself the author of the first important separate work published on the butterflies of Japan, which is also noticeable as having been issued in two languages, English and Japanese. On the other hand, there are comparatively few species and specimens from North Africa and Western Siberia.

Mr. Leech also interested himself specially in the variation of species, and purchased a large selection of varieties of European Lepidoptera from the collection of the late Herr Mützell, of Berlin, as well as from other sources; and as the types of new species in Mr. Leech's collection have already been fully illustrated in the works and papers published by Mr. Leech himself during his lifetime, the two plates which illustrate the present memorial volume are devoted to figures of some of the most interesting varieties, chiefly European. Every specimen in the collection is carefully enumerated in the volume before us, the sex and exact locality being carefully indicated, and all types marked.

Entomologists owe a deep debt of gratitude to Mr. Leech himself, to the liberality of his mother, and to the careful work of his friend and coadjutor, Mr. South, in ensuring the permanent value of this unique collection.

Bacteria in Daily Life. By Mrs. Percy Frankland. Pp. 216. (London: Longmans, Green and Co., 1903.) Price 5s. net.

Mrs. Frankland has compiled an interesting, instructive, and accurate account of the modern developments of bacteriology. Such subjects as sewage disposal, the prevention of tuberculosis, micro-organisms in milk, air, and foods, which are of public importance, are fully dealt with, and the modern ideas regarding toxins and antitoxins are briefly discussed. No one nowadays laying claim to a liberal education can dispense with a slight knowledge, at least, of microbes and their actions, and for such this work will prove an adequate text-book. R. T. Hewlett.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

A New Theory of the Tides of Terrestrial Oceans.

In Nature of September 4, 1902 (vol. lxvi. pp. 444–445), Prof. G. H. Darwin makes some criticisms upon a paper of mine to which I should like to reply.

Upon referring to pp. 537 and 624 of the paper criticised, it will be seen that it aims at "rude approximations to the cases found in nature," and at a "partial explanation of the tides." In fact, it bears the title, "Manual of Tides, Part ivA., Outlines of Tidal Theory." If, therefore, the paper establishes, even in a few cases, the principal causes of the tides, connecting the latter with the known tidal forces, it can hardly be regarded as a "failure," even though the approximations are rather rough; for I believe this object has not been heretofore attained for any ocean tide, although statements have elsewhere been made by our critic which might, perhaps, lead some people to think otherwise.[1]

Again, granting for the moment that the theory involved in the paper is erroneous, I should still say that if observed facts can be conveniently grouped by aid of it, a useful purpose will have been subserved. In fact, the mere collection of tidal data which a test of any theory implies is here, as elsewhere, not without value. For instance, if our critic could have had this paper before him while preparing his book on tides, he would not have overlooked Berghaus's invaluable cotidal chart and written "No more recent attempt (than Airy's) has been made to construct such a map."[2]

Prof. Darwin's principal criticisms are three in number:—

(1) He sees no use for the equation of virtual work in ascertaining the times of high water.

(2) He thinks that the deflecting force of the earth's rotation cannot be generally disregarded in a first approximation, which is all that my paper aims at.

(3) He does not believe that ocean basins exist the free periods of which are sufficiently near the tidal period to account for the tides.

(1) Concerning my application of the principle of virtual work, Prof. Darwin is mistaken when he says "Mr. Harris takes the displacements as proportional to the actual displacements per unit time." What is really done is this:— The magnitude of the virtual displacement (δx, say) at any given point of the system is taken to be the same for any given time or hour, but varies from point to point. Since the law of the oscillation of the particles is known, viz. it is simply harmonic in time, and the particles throughout the body are at a given instant in like or opposite phases, the virtual displacement at any given point may always be represented by the maximum value of the actual total displacement at the point (*cf.* rule quoted in criticism). In other words, if we choose to consider the small virtual displacement as identical with a small actual displacement corresponding to a time variation, the implied δt will not be constant for all hours. Hence the virtual displacements at different hours are not simply proportional to the actual displacements *per unit time.* He is evidently mistaken when he says, "Thus all sustaining forces vanish at the instant when the displacement is a maximum." Why should they? Surely they generally vary in magnitude and phase for the various parts of an extended oscillating body. Probably the use of the rule quoted in the criticism and founded upon the principle of virtual work can be most readily seen when it is applied to a binodal canal-like area of uniform cross section, selecting for simplicity, say, the nodes as the points of application of the sustaining forces (*cf.* § 63). The process implied in the rule seems to be correct, and, so far as I see, about as simple as it could

[1] "The Tides," p. 177, lines 2–10. [P. 160, lines 16–23, English edit. I thought that the passage referred to would be understood to refer to the ideal case there under consideration.—G. H. D.]

[2] "The Tides," p. 189, lines 10–12. [P. 171, lines 19–21, English edit. This was an oversight; a reference to Berghaus will be found in the forthcoming article on the tides for the German "Encyclopædia of Mathematics."—G. H. D.]

have been made. Further on the criticism reads, "I fail to see any adequate consideration of the variability of depth, of the absence of synchronism in the disturbing force in the direction of the canal." This "absence of synchronism" is precisely what the criticised equation 308 (or 311) enables us to take account of.

It seems to me that enough has been given in §§ 38, 42, 45, 55, and 63 to show that the variability in depth has not been permanently lost sight of, and also enough to convince one that "areas" as nearly uniform in depth as are many portions of the ocean can, as a first approximation, be treated as bodies having strictly uniform depths.

(2) Of course there are instances where the deflecting force due to the earth's rotation becomes important; for example, most moderately narrow arms of the sea in which the current is swift—such as the English Channel, Irish Channel, and Gulf of Georgia. But if in any of these a large stationary wave actually exists, it is hard to see how the times of its high and low waters near the loops can be seriously affected by this force, and these are the only times which chapters vi. and vii. undertake to determine. Near the nodes, when the current is swift, the deflecting force may, in a canal the width of which is but a moderately small fraction of a half wave-length, cause high water at one end of the nodal line, and at the same time low water at the other. This is true because the narrowness of the body permits its transverse slope to respond at once to the transverse forces. A progressive wave can be so superposed as to diminish or even destroy the range at one end of the nodal line while increasing the range at the other end.

Considering now a broader "area," with one or both of its lateral boundaries wanting, it is hard to see how the transverse motion occasioned by the earth's rotation can seriously interfere with the character of the stationary wave, and especially the time of elongation of the particles; for its effect cannot accumulate and so tend to produce a transverse stationary oscillation. If, on the other hand, a square or rectangular "area" about half a wave-length wide have solid lateral boundaries, it would seem that the deflecting force might, except in the equatorial regions, so alter the mode of oscillation that it could not be ignored even in the first approximation. So far as I know, there is no near approach to this case in any of the "areas" which probably exist (see Fig. 23 of my paper).

Hence, while it is true that the free oscillations in a rotating rectangular sheet of water is an unsolved problem, we see that the critic's remark, "It seems to follow that either Lord Kelvin or Mr. Harris is wrong," if in any sense true, really has very little to do with the case. In a word, taking an oscillating body as a whole, it seems to me that the oscillation, in accordance with a simple mode, can generally be regarded as the fundamental and important thing, and the effect of the earth's rotation a modifying or induced phenomenon.

(3) Now in regard to the improbability "that any large portion of our curiously shaped oceans should possess even approximately the critical free period," several things can be said. In the first place, we are not restricted to *single* half wave-lengths; the rectangular "areas" may run in any direction; the "areas" may be approximately trapezoidal, triangular, or of other forms, their free period may differ perhaps 10 per cent. or more from the period of the forces, and still have their tides greatly augmented by their approach to critical lengths. There are, indeed, portions of the ocean which cannot be covered by any areas the periods of which would be satisfactory, and in which it would be possible for the tidal forces to incite a considerable tide. Upon referring to the map, Fig. 23, it will be seen that one such region exists west of Australia, another south of New Zealand, another east of southern South America, the Arctic Ocean constitutes another. Upon referring to the map of the diurnal tides, Fig. 24, it will be seen that the South Atlantic, the South Pacific, and all of the Arctic Ocean are not regions where we can reasonably expect to find large diurnal tides.

Referring again to Fig. 23, and noting that the ocean is for the most part actually parceled out into areas of considerable width the free periods of which can hardly differ greatly from twelve lunar hours, and are, moreover, so situated that the forces do not approximately destroy one another, as can be seen by applying the rule quoted in the criticism, it may, perhaps, be justifiable to ask how it happens that the times of high and low water at the loops, as determined by this rule, do approximately agree with observed times, unless there is some considerable truth in this "partial explanation of the tides."

Recently I have been working out in considerable detail the tides in the equatorial belt of the Indian Ocean, where it is fair to assume that the effect of the deflecting force must be small. The work goes to show that the theory set forth in the criticised paper is substantially correct. I therefore venture to refer Prof. Darwin to this discussion, which will appear in the March number of the *Monthly Weather Review*.

To avoid needless misunderstanding, it may be added here that I am well aware of the incompleteness of the treatment given in my paper. For instance, mathematicians have not up to this time been able to treat the simple problem of a rectangular "area" the rigid boundary of which consists of only two opposing end walls, although much has been done upon analogous problems relating to the open organ pipe. Even an approximate absolute value of the range of tide (excepting in small deep bodies) has not been attempted in this paper, because its determination would involve the numerical value of frictional resistance, which can be kept in abeyance when we seek only the times of tides in systems which have as free periods very nearly the tidal period. Many deductions and refinements were purposely omitted from my paper—the chief aim being simplicity. I hope eventually to be able to consider more fully matters like these in connection with detailed studies of the tides in various seas. R. A. Harris.

Washington, D.C., March 28.

March Dust from the Soufriere.

Sir W. Thiselton-Dyer has kindly forwarded to me a packet of volcanic dust sent to him by Dr. D. Morris, which fell in Barbados last month after an eruption of the Soufriere of St. Vincent, a brief description of which may be of interest. The sample, Dr. Morris states, was collected at Chelston, Bridgetown, on sheets laid out upon the lawn, the material being brought in and weighed every hour, and the fall continuing from 11 a.m. to 5 p.m. on the day of the eruption. It is free from all extraneous matter, and may be regarded as typical of the ash which fell on Barbados. The weight of this is estimated at about 6000 pounds (avoir.) per acre. At an average rate of three tons per acre, this would be equivalent to about 300,000 tons for the whole island.

The dust is of a dull dark brown colour, showing on close examination a minute speckling with a lighter tint. If poured on a piece of white paper and removed in the same way, a distinct warm-brown tint remains, produced by the very finest part of the powder, which is not easily removed. In Dr. Flett's excellent account of the dust which fell in Barbados after the eruption of May 7 (*Quart. Jour. Geol. Soc.*, lviii., 1902, p. 368), it is stated that this was at first brown, then slightly redder, and at last a whitish-grey impalpable powder. A bulk sample of that fall is distinctly greyer than the recent one, and a small one of the fall of 1812, in my possession, is a rather pale grey with a slight brown tinge. The new sample under the microscope differs only in detail from that described by Dr. Flett. The fragments, as a rule, do not exceed 0·01 inch, and are thus very slightly smaller than some in the May eruption; from 0·06 to 0·08 is a rather common size, and there is a fair amount of exceedingly minute dust. The principal minerals are the same, plagioclastic felspar, hypersthene, and a green augite, but in the first steam cavities are now more abundant than glass enclosures, and I think brown glass is more often adherent, but to make certain of this point requires a fuller examination than I can give for the next few days. T. G. Bonney.

The Lyrid Meteors.

The Lyrid meteors excite an interest that might be regarded as quite disproportionate to their numerical importance. They are a very rare shower, and even when considered by experienced observers as unusually abundant, they seldom appear at a higher rate than about twenty per hour.